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Discussions

Interactive comment on "Climate change impacts on the seasonality and generation processes of floods in catchments with mixed snowmelt/rainfall regimes: projections and uncertainties" *by* K. Vormoor et al.

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GENERAL COMMENTS

The paper addresses the impact of climate change on flood seasonality and related changes in flood generation processes for selected catchments in Norway. It is a thorough study in the sense that it uses a multi-ensemble approach to assess the uncertainty in model simulations and future projections, more specifically eight different regional climate models (RCMs) and two different downscaling methods. Only one hydrological model is used, although 25 different parameter sets are included to account

for parameter uncertainty. The IPCC-AR4 scenario (C20 for the current period) has been chosen along with the SRES A1B scenario for the future. The study investigates both changes in magnitudes and frequency of events and includes a valuable section on sources of uncertainty.

The topic is relevant and important and the paper reports some interesting results; however, it provides limited new knowledge or insight into what is a much studied topic. Aspects to consider in this respect are:

1) It is well known that there will be a shift in flood seasonality due to climate warming, from snow dominating floods to more rain controlling floods in regions with a seasonal snow cover and accordingly, a change in controlling processes (e.g. J. Parajka, 2010). However, as also mentioned in the paper, precipitation is projected to increase in the region as already documented, particularly on the western coast. Thus, it is important to account also for changes in seasonal precipitation when discussing changes in flood seasonality. The paper briefly mentions this aspect, however, it is recommended that is also include a quantitative analysis of changing (seasonal) precipitation and temperature pattern to better distinguish the relative importance of increasing temperature versus changes in precipitation. It would further have been of interest to assess any trends in the observed period and compare these with future predictions.

2) The use of the AR4 scenarios rather than the CMIP5, makes the study somewhat outdated (although the main conclusions may not change that much).

3) The use of only one (conceptual based and calibrated) model in (what is likely) a non-stationary climate should be commented on, and more general, the role of hydrological model uncertainty in climate change impact studies (e.g. Velázquez et al., 2013; Bosshard, et al., 2012).

4) The use of only six catchments and their location. It is noteworthy that the selection does not include a catchment in western Norway, which is specifically mentioned as an area of interest due to high precipitation rates (ref. Introduction). This is also a region

where precipitation is projected to increase significantly in the future (and already has).

5) The topic of the study lends itself to a regional study and six catchments is a rather low number given the high hydroclimatic variability across Norway. Only with a better coverage can one conclude on regional patterns and trends in flood patterns (in the current as well as future climate), as these can vary considerable locally. This can be achieved either by increasing the number of catchments or by using a gridded dataset for Norway (e.g. data from seNorge.no, which contains both interpolated climate and simulated runoff based on a gridded version of HBV). The current study design is in my opinion not sufficient to conclude on regional patterns in flood seasonality (refer Objective 1). Accordingly (provided that the study is not extended), the conclusions must be revised to be more catchment specific and less general.

6) Objective 3 can only be answered if the role of changing precipitation and temperature patterns are included explicitly (ref. point 1 above).

7) When objective 4 is presented, we have not yet been informed about the different ensemble components. The latter aspect needs to be better introduced, including the design of the modelling strategy. Section 3.1. says what it consists of, but not why this particular design was chosen. Perhaps it is partly what is said on p.6286, line 10: "identify the fractional uncertainty emerging from different sources within the model chain for three variables: . . ."

SPECIFIC COMMENTS

a) The introduction gives reference to various trend studies (in observations), but not to particular studies on trends in floods, which should be added.

b) The result of the paper should be discussed in light of similar studies, and not be limited to national (or Nordic) studies. Also pan-European trend studies would be of interest as well as studies from similar regions in other continents (e.g. U.S. and Canada).

c) Reference should be made to existing regime classifications for Norway (here only

two regimes classes are suggested). Other regime classifications distinguish more classes and could also be used as a starting point for selecting representative catchments.

d) The study proposes a seasonality index, SD, and separates between two seasons. In this respect, the authors should clarify:

i. Why is the second term in the index included (does it add any information)?

ii. The first term describes the ratio between the flood peaks in m-3s-1; does this mean that you sum the POT discharge values?

iii. Is it valid to use the same two seasons for all catchments given their high variability in hydroclimatic regime (and will they be representative in the future)? iv. How will the use of a fixed threshold (here the 98.5 streamflow percentile) influence the selection of events if there is a change in annual precipitation (and thus streamflow) in the future?

v. How is the normal flood duration defined? Is there a different value for snow generated events as compared to rainfall (different response times)?

vi. Present and argue for your proposed seasonality index in light of existing definitions (e.g. J. Parajka, 2010).

e) The classification into three flood generation types is based on the contribution of rain and snow to the runoff. What about rain on snow events; how would these be classified based on the HBV model simulations?

f) Combining the result and discussion section can be challenging. Here, the results are discussed under specific headings, which is fine. However, this requires an overall discussion bridging between the different sections (option to add such a section at the end of the combined section).

g) It is concluded that the relative role of hydrological parameter uncertainty is highest in catchments showing a high change in flood seasonality. Is this not just a result of high model sensitivity to the threshold temperature (snow/rain and melt/no melt), implying a widely different response in runoff to small changes in temperature?

h) The abstract needs to better represent details of the study, e.g. number of catchments, multi-model in what sense, what are the ensemble components?

i) The abstract reads “Changes towards more dominant autumn/winter events correspond to an increasing relevance of rainfall as a flood generating process (FGP) which is most pronounced in those catchments with the largest shifts in flood seasonality. Here, rainfall replaces snowmelt as the dominant FGP”. Later it is stated (Section 4.4) “Rainfall becomes the dominant FGP in the future period in all investigated catchments”. There is here a need to distinguish the relative contribution of a precipitation increase (rain or snow) vs. a shift in precipitation from snow to rain due to a temperature increase. In other words; what is the role of increasing temperature vs. changes in precipitation patters for the different catchments (should be evaluated on a seasonal basis). Ref. point 1 under General comments.

j) An important observation, although a bit hidden, is given in Section 4.4, p.6290, line 21: “the rainfall-generated POT events tend to occur later in the year”. This should be further elaborated and possible reasons discussed.

k) It is argued that the selection of only two classes is chosen to obtain a broad picture of flood seasonality. Why not simply look at changes in the flow regimes, i.e. changes in the month of the highest peak? This would allow you to analyse a more general shift in flood occurrence, not restricted by the choice of a fixed season (temporal as well as spatially).

l) It is mentioned that the HBV snow and melting module has a semidistributed structure. More details are here needed as the formulation of the snow routine is vital for the study, e.g. what is the spatial resolution of the elevation zones, how is the climate input interpolated to different elevation zones, how is snow melt calculated?

m) Is the RCM downscaled to the scale of the catchment area or to a gridded structure? and how is the climate input distributed to the different elevation zones? More details needed.

n) The reference to ‘equifinality’ should be deleted as I cannot see that the work specifically addresses this aspect; instead focus should be on parameter uncertainty only.

o) The last paragraph of Section 3.5.2 is not clear. What is the ‘flood duration time of the core event’ and what implication does it have that the duration is extended by adding ‘the catchment specific recession time’.

p) Section 4.3 is important, but the approach (changes in magnitudes vs. the frequency of events) has not been well introduced in the Method section.

q) Figure 2: comment also on the spread, not only on the median.

r) Figure 4: add the observations to the seasonal plot.

s) Figure 5: Is this result based on an average across the model ensemble for all 25 parameter sets?

t) Figure 6 needs a better introduction (hard to read and not well explained). Difficult to understand the text that follows (p.6291, line12-20), and this section needs revision.

TECHNICAL CORRECTIONS

i. P.6275 (line 21). The reference by Lawrence and Hisdal (2011) cite change in flood frequency, then refers to flood magnitudes; please clarify.

ii. P.6227, line 17: rewrite as i. reads like snowmelt in inland and northernmost Norway causes high flow s during spring and summer in the whole of Norway (similar for ii.).

iii. P.6282, line 6: ‘this approach performs remarkable well’; provide details of what performs well and where.

iv. Overall use comma more (particular to distinguish between the use of ‘that’ and ‘which’).

v. Suggest to replace the word ‘mismatch’ when discussing model performance with something more informative, e.g. underestimation, . . .

vi. P.6287, line 21: Sentence starting: “For Fustvatn”, is this the correct catchment here?

vii. P.6293, line 4: replace “different regions” with “six catchments representing different . . .”

REFERENCES

Velázquez et al., 2013: An ensemble approach to assess hydrological models’ contribution to uncertainties in the analysis of climate change impact on water resources. HESS

Bosshard, et al., 2012: Quantifying uncertainty sources in an ensemble of hydrological climate-impact projections, Water Resour. Res., doi:10.1029/2011WR011533,

J. Parajka, 2010: Seasonal characteristics of flood regimes across the Alpine–Carpathian range. Journal of Hydrology, 394, 78–89.

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